shape of even a condensed account of the matters specially selected by Dr. Garrod, and so one need only mention that the anomalies treated at length are albinism, cystmaria, alkaptonuria, and pentosuria.

It must not be supposed that the examination of these comparatively rare conditions is devoid of general interest, for it is often just these curious accidents of perverted tissue change which form the opportunity of the keen observer in unravelling the perplexities of the normal state. The natural sequence of study is physiology first, pathology next. But very often an inversion of this order leads to important accessions to knowledge. Dr. Garrod is to be congratulated on having been successful in such an experiment, and those interested in metabolism cannot do better than study his lucid and bright exposition of the subject. W. D. H.

Practical Testing of Gas and Gas-meters. By C. H. Stone. Pp. x+337. (New York: John Wiley and Sons; London: Chapman and Hall, Ltd., 1909.) Price 15*s*. net.

THIS is a laboriously complete compilation of the various methods of testing gas for illuminating power, purity, chemical composition, and calorific value, and of proving the accuracy of the indications of gas-meters. The subject is one mainly of technical interest only, and very specially so even for that, and hardly suitable, therefore, for detailed consideration in these columns. An examination of the book shows how great a diversity of apparatus has been devised and thrust upon the innocent gas-producing world, and how gratuitous some of the diversity is. Where apparatus has been designed for official testings, the objection to protecting the manufacture by patents has given the constructor liberty to alter and "improve" or spoil an instrument, as the case may be.

The American has a great opinion of the English official ten-candle lamp designed by Mr. Harcourt, but he will not take it as he finds it, and so he makes an American pattern. The English official calorimeter, too, judging by the observations made, has also gone through a metamorphosis in crossing the Atlantic. As is to be expected, the book is well got up and illustrated, and its value is increased by the inclusion of a number of tables of value to those whose business is to test and examine gas.

A Compendium of Food-microscopy. By E. G. Clayton. With sections on Drugs, Water, and Tobacco. Compiled, with additions and revision, from the late Dr. A. H. Hassall's works on Food. Pp. xxxix+431. (London: Baillière, Tindall and Cox, 1909.) Price 10*s*. 6*d*. net.

THIS book, written by an ardent disciple of the late Dr. Hassall, is largely based on Dr. Hassall's works on food and its adulteration. An account is given of the microscopical characters of all the principal vegetable food-stuffs, beverages such as tea and coffee, fruit preserves and condiments, and of tobacco, opium, and a few other drugs, together with those of the chief adulterants of these substances. In addition, chapters are devoted to foods of animal origin and the parasites which may infect them, milk, cream, butter, and cheese, and to the microscopical flora and fauna of water. The book is profusely illustrated with line drawings to scale, which usually reproduce very clearly the characteristics of the substances they are intended to depict, though occasionally there is an irritating want of systematic arrangement of the figures, Fig. A, for instance, sometimes being on the right, sometimes on the left, of the page. Though the botanical terminology is not always that used nowadays, on the whole the book should form a useful addition to the library of the analyst and microscopist.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Molecular Scattering and Atmospheric Absorption.

SINCE Lord Rayleigh discussed the question of molecular scattering, and its bearing on the explanation of the blue colour of the sky, our experimental and observational data have become much more trustworthy. While our knowledge of the number of molecules in a gas allows us now to calculate with sufficient accuracy the amount of direct sunlight which is diverted by scattering, Mr. Abbot's series of measurements at Washington and on Mount Wilson gives us the actual observed opacity of the air for different wave-lengths.

Lord Rayleigh showed that, on the hypothesis of the elastic solid theory of light, small particles of matter, which act simply by adding inertia to the æther, scatter light and retard the passage of a wave passing over them in such a way that the relation

$$k=\frac{32\pi^3(\mu-1)^2}{3N}$$

holds, where k is the coefficient of extinction of energy, μ the refractive index, and N the number of molecules per cubic centimetre. He showed, further, that the same equation may be deduced from the electromagnetic theory if the particles locally affect the inductive capacity of the medium. In the forthcoming new edition of my "Optics" it will be proved that the equation is independent of theory, provided μ is nearly equal to unity; the limit of its applicability is only reached when there is a retardation of phase at the origin of the scattered light the square of which is appreciable, and it can be shown that this is actually the case except within the region of anomalous dispersion. The range of the formula may be further increased if $\frac{1}{4}(\mu^2-1)^2$ is written for $(\mu-1)^2$.

For N I have used Rutherford and Geiger's value $2{\cdot}72\times10^{19}$, and with the known value of the refractive index of air, k may be calculated. If h is the height of the homogeneous atmosphere above the point of observation, e^{-kh} is the fraction of light which would reach the observer if no light were lost in any other way than by molecular scattering. In the following table the transmitted light calculated in this manner is compared with Abbot's observed figures. The first column gives the wave-length, the second column contains the observed values of the transmitted energy for Washington, taking all observations into account, while the third column gives the number calculated from the observations on February 15, 1907, when the air was exceptionally clear. The calculated values are entered into the fourth column. The last three columns give the corresponding numbers for Mount Wilson. The selected clear day in this case was October 11, 1906.

	Washington			Mount Wilson		
Wave-length	Observed mean	Observed clear day	Calculated	Observed mean	Observed clear day	Calculated
4×10^{-5}	0·55	0·72	0·71	0·73	0·76	0·76
5	0·70	0·84	0·87	0·85	0·89	0·89
6	0·76	0·87	0·94	0·89	0·92	0·95
7	0·84	0·90	0·96	0·94	0·96	0·97
8	0·87	0·94	0·98	0·96	0·99	0·98
10	0·90	0·96	0·99	0·97	0·99	0·99

The close agreement between the two last columns shows that on a clear day on Mount Wilson atmospheric absorption is practically accounted for by molecular scattering. There is a slight indication of selective absorption in the red, but otherwise the columns are in complete agreement. On the average day there seems an additional absorption of about 2 per cent. It is remarkable that, even at Washington, the calculated absorption for blue light should so nearly agree with the calculated value; this means that even at the sea-level the greater part of the absorption on a clear day is due to scattering by the molecules of air. The large diminution in the

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intensity of blue light at Washington on the average day seems to indicate that there is a substantial amount of scattering by small solid or liquid particles.

The figures for Mount Wilson give us confidence in the trustworthiness of Mr. Abbot's determination of the solar constant, because it is clear that the total effect of the atmosphere can be eliminated with greater certainty if it is mainly due to the permanent constituents of the atmosphere, and not to matter which is variable in amount. As the top of Mount Wilson is less than 1800 metres above sea-level, we may conclude that at high elevation the blue colour of the sky is completely accounted for by molecular scattering.

That the scattering sets a limit to the transparency of gases must be kept in mind in discussing problems of solar and stellar physics. We may feel confident, for example, that what is called the reversing layer can only have a small thickness, for otherwise we should not be able to observe so far into the ultra-violet as we do.

The scattering may profoundly modify the character of the spectrum, as I have explained in a paper on "Radiation through a Foggy Atmosphere" (*Astrophysical Journal*, vol. xxi., p. 1), in which it is shown how increased thickness, without change of temperature, may convert an absorption line into a bright line. It seems to me probable that the bright and dark flocculi shown in spectroheliograph pictures may find their explanation chiefly in variation of thickness in the absorbing layer, the bright patches being due to increased thickness.

The transparency of interstellar space has recently received a much needed critical discussion at the hands of astronomers, and Prof. Turner has applied the law of scattering to explain certain discrepancies between visual and photographic magnitudes. The value he gives for the opacity allows us to calculate the average density of the matter which is diffused through space on the supposition that it is gaseous. If the value of $(\mu-1)/D$ (where μ is the refractive index and D the density) be taken to be approximately equal to that of air, I find that the number of molecules per cubic centimetre in space would have to be of the order of a million, and the mean free path of the order of 3000 kilometres.

Although not directly connected with the subject which forms the main part of this communication, I would like to point out that the same analysis which gives the coefficient of extinction in terms of the retardation of phase at the source of the scattering also gives a resultant force acting on the molecule in the direction in which the light is passing. When summed up for all the molecules this force is found to be identical with what is generally called the "pressure of light," for if E represents the energy density, the force acting per unit volume on the scattering molecules is found to be kE, where k is the coefficient of extinction.

There is a widespread impression that light pressure only acts on particles the linear dimensions of which include several wave-lengths of light, but this is not correct. The determining factor is the extinction of light, whether by scattering or by absorption, as indeed appears if we take the view adopted in Prof. Poynting's work on the subject that a propagation of momentum accompanies the transmission of light. The momentum is destroyed equally whether the molecules act as scattering or as absorbing centres. The extinction by scattering near the surface of stellar bodies does not, however, appear to be sufficient to cause any measurable effects comparable with their gravitation. ARTHUR SCHUSTER.

The Fixation of Nitrogen by Soil Bacteria.

MAY I be allowed through the columns of NATURE to ask Prof. W. B. Bottomley a few questions with regard to his paper on "Some Effects of Nitrogen-fixing Bacteria on the Growth of Non-leguminous Plants" (Proc. Roy. Soc., B, lxxxi., 1909, 287), abstracted in NATURE of May 13 (vol. lxxx., p. 327), as I had not the opportunity of being present when the paper was read?

Prof. Bottomley bases his conclusions on experiments to show that Pseudomonas, the bacterium associated with the leguminous plants, will fix more nitrogen in an artificial culture, when Azotobacter is also present, than when alone. He quotes the following results:—

Control	0·48	mgm. N. per 100 c.c. culture solution
Pseudomonas alone	0·91	,, ,, ,,
Pseudomonas and Azotobacter ...	1·24	,, ,, ,,

These differences would almost seem to be within the range of experimental error, but in any case, does not the demonstration require a further statement of how much nitrogen Azotobacter alone would fix? Other observers are accustomed to get fixations by Azotobacter alone of from 5 to 20 mgm. of nitrogen per 100 c.c. of such a culture solution, the maximum being about 10 mgm. of nitrogen fixed per gram of mannite. The only conclusion that could be drawn from Prof. Bottomley's figures would be that Pseudomonas injuriously affects the power of Azotobacter to fix nitrogen, supposing that a reasonably active culture of the latter had been used.

Turning to the field experiments, in which Prof. Bottomley claims to get an increase of crop by adding cultures of Pseudomonas and Azotobacter to soil which presumably already contains both organisms, data for estimating the probable experimental error are again lacking. From the Rothamsted experiments, where we may assume the conditions are more than usually favourable to exactitude, the mean error of a pair of similarly treated plots in a single year is about ±10 per cent., which would more than cover the differences observed by Prof. Bottomley's experiment with oats.

In another experiment with barley, Prof. Bottomley obtained a higher percentage of nitrogen in the corn from the treated strip than in the corn from the rest of the field, 1·76 against 1·55 per cent. (I presume that "milligrammes of nitrogen per cent." is a clerical error). In view of the comparatively small changes in the composition of the grain of a cereal which are usually effected by large variations in the supply of nutrient, I should like to know from Prof. Bottomley if duplicate samples were taken from different parts of the untreated section of the field, and what range of variation they showed in their nitrogen content.

Coming to the next experiment, it is difficult to judge how far a bulbous plant like *Galtonia candicans* is suitable for experiments on nutrition, but it is rather necessary to know what relation the weight of the bulbs planted bore to those harvested. Prof. Bottomley only says that 250 bulbs "of equal size" were planted in each bed. Can he let us have the weights in each case? Moreover, he tells us that the treated bed was twice watered with the culture solution, the control bed being given pure water at the same time; was the same amount of water given to each, and how much of the culture solution was applied, for it contained monopotassium phosphate, sodium chloride, &c., which may well have been a considerable factor in any beneficial effect experienced?

Prof. Bottomley will perhaps forgive me if these questions may seem somewhat critical of his conclusions, but any communication appearing in the Proceedings of the Royal Society must be taken into account, and one therefore wishes to have the data necessary for determining the weight to be attached to the results. A. D. HALL.

The Rothamsted Experimental Station, July 12, 1909.

Occasional Unexplained Ringing of House-bells.

AN observation sent me by Mr. Alexander Sinclair, of Swansea, to the effect that during a thunderstorm drops of water leaking through the ceiling "assumed a pear shape and jumped 9 inches almost horizontally to the curtain rings above the window," suggests that house-bells of the ordinary non-electric type may occasionally be rung by this means. I picture the process as follows:—The bell wires collect atmospheric electricity, by induction or otherwise, which the walls are insufficiently conducting to carry off freely; consequently the bells get charged, are attracted to a neighbouring wall or pipe, and released suddenly by a spark. This little lateral jerk rings the bell.

I put the simple suggestion on record because I sometimes hear of an inclination to attribute the phenomenon to less familiar causes. OLIVER LODGE.

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